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## CULTURAL NEGOTIATIONS

CRITICAL STUDIES IN THE HISTORY OF ANTHROPOLOGY

Series Editors: *Regna Darnell, Stephen O. Murray*

# Cultural Negotiations

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## *The Role of Women in the Founding of Americanist Archaeology*

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DAVID L. BROWMAN

*University of Nebraska Press | Lincoln and London*

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Library of Congress Cataloging-  
in-Publication Data

Browman, David L.

Cultural negotiations: the  
role of women in the founding  
of Americanist archaeology /

David L. Browman.

pages cm.— (Critical studies in  
the history of anthropology)

Includes bibliographical  
references and index.

ISBN 978-0-8032-4381-1

(cloth: alk. paper) 1. Women

archaeologists—Biography.

2. Archaeology—United

States—History. 3. Women

archaeologists—History.

4. Archaeologists—Biography.

I. Title.

CC110.B76 2013

930.1092'2—dc23 2012049313

Set in Lyon by Laura Wellington.

Designed by Nathan Putens.

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## *Series Editors' Introduction*

REGNA DARNELL AND STEPHEN O. MURRAY

David Browman has produced an invaluable reference work for practitioners of contemporary Americanist archaeology who are interested in documenting the largely unrecognized contribution of generations of women to its development. Meticulous examination of the archaeological literature, especially footnotes and acknowledgments, and the archival records of major universities, museums, field school programs, expeditions, and general anthropological archives reveals a complex story of marginalization and professional invisibility, albeit one that will be surprising neither to feminist scholars nor to female archaeologists. This trend is consistent over the nearly eight decades that Browman explores, between the end of the American Civil War in 1865 and 1940, just prior to the vast postwar expansion of academic archaeology in North America. Only a few of the 148 women are familiar names. Minor figures abound as they do not in the history of cultural anthropology, generally a more solitary enterprise, and they give a peculiar and unique flavor to the professionalization of American archaeology.

By identifying otherwise unrecognized women, this work sets a new standard of evidence for feminist archaeological historiography. The tracing of changing surnames reflecting successive marriages is especially valuable because many of the contributors were not previously identified as the same women. For example, C(atherine) Allison Clement [Withers] [Paulsen] used her middle name and was married twice; or Theodora Kracaw [Brown] [Kroeber] [Quinn], the second of whose



three marriages was to cultural anthropologist Alfred Kroeber and who was better known as a folklorist than as an archaeologist; or the never married but nonetheless extensively named Frederica Annis Lopez De Leo De Laguna. Interspersed with the biographies and archaeological activities of these nearly forgotten predecessors are more extended evaluations of some of the rare women who “made it.” The discussion of Gene Weltfish is particularly instructive. Biographical vignettes are uneven, partly because of available evidence and partly as a result of the materials consulted over the huge range of possible materials for this study.

It is refreshing to see a male archaeologist address the problem of marginalized women in archaeology given that exclusions and discrimination are usually felt to entail some degree of complicity. Nonetheless, naming the problem is a step toward the future archaeology Browman envisions, in which gender will no longer determine the career trajectory of women in the field. Many of the women described here disappeared from archaeology but went on to distinguished careers in other fields (seemingly a source of some disappointment to Browman). During most of the period covered, the simplest way for a woman to become an archaeologist was to marry one, a route taken by many.

Cross-reference is possible by culture area as well as individual, so readers can search for links to their own interests and specializations. The extensive bibliography shows where searches have been made and suggests directions for further investigation. Browman’s research expands outward from Harvard University and the Peabody Museum of American Archaeology and Ethnology, the home base of his own career and most intensive archival investigation; future investigators doubtless will choose to pursue other tendrils of influence and participation. Documentary methods differ, however, depending on the decade in question because of the rapidly changing institutional basis of American archaeology during this 75-year period. In more recent decades, the number of women increased, and Browman’s more stringent criteria for inclusion reflect increasing professionalization.

Some generalizations emerge, although the foremost purpose of this volume is documentary rather than analytic. Professionalization operated against women because it was harder for them to obtain the graduate credentials that were increasingly necessary for academic employment. The role of widows who came to control their own wealth as philanthropists who supported archaeology and encouraged women's participation is especially fascinating: for example, Phoebe Apperson Hearst in California, Sara Stevenson in Philadelphia, Zelia Nuttall in Cambridge, and Mary Wheelwright and Elsie Clews Parsons in the Southwest. Edgar Hewett and Frederic Ward Putnam emerge as patrons of women in archaeology and facilitators of their participation, albeit often in minimally prestigious roles as lab assistants rather than active fieldworkers. The unabashed chauvinism of other male archaeologists is also noted, without apologetics.

There is much more that might be said, but Browman's book gives a clear starting point for documentation and invites interpretive scholarship across disciplines ranging from archaeology to feminist research to cultural studies. Such is the process of a critical history of anthropology!



## CULTURAL NEGOTIATIONS



## *Introduction*

This volume concerns cultural negotiations by women in the United States as they sought to secure access to training and jobs in Americanist archaeology as the discipline emerged. I use the term “Americanist archaeology” to delimit archaeology done in the Americas, not archaeology done by Americans who have worked worldwide. Because the focus is on women in this field and the issues they had to confront, my treatment omits any comparable coverage of problems facing men during this same period (1865–1940). A few women turn up in general histories of Americanist archaeology, but most of them have been missing from previous accounts.

My temporal focus is on the women involved in Americanist archaeology from the end of Civil War until the beginning of World War II. Both wars caused significant shifts in the economic and social status of women in American society and the opportunities thus available to them. Before 1860, for all intents and purposes, women were not involved in Americanist archaeology. The tremendous manpower drain of the Civil War resulted in women taking over jobs that men had traditionally filled. Women were able to benefit from this situation by continuing on after the war in positions opened up to them by the conflict.

There was also a significant shift in opportunities after the First World War, but involvement of Americans in that war effort was significantly less than either the Civil War or the Second World War. Consequently the First World War did not have as striking an impact on the situation

of women in American society and academia. The end point of the discussion at 1940 marks another more definite paradigm shift. Contributions by Americans of all sexes, ethnic groups, and economic levels in World War II had a dramatic impact on the postwar educational and professional reorganization and opportunities, the particulars of which are beyond the scope of this volume. Thus in the second half of the twentieth century, various patterns noted in these pages often came to an end or evinced a new slant due to the social impact of women's contributions and other related events of the Second World War.

In the United States there were different developmental trajectories for classical/humanistic archaeology as contrasted with social science/anthropological archaeology after the 1870s. Because of the humanistic, linguistic, and philological bents of classical archaeology, at least in the classroom, women had different experiences than those encountered in anthropological Americanist archaeology.

Why should I care about these intellectuals? One answer frequently given to this query is that because of sexism in historiography of the discipline, women need to be rescued from obscurity. How significant is this issue? Nancy Lurie (1999/1966:2) argues, "As it turned out, early women have been relegated to no more obscurity than have many of their male contemporaries."

But in reviewing the intellectual histories of the Americanist archaeological discipline, it is telling that none of the 1950s and 1960s syntheses such as those by J. O. Brew (1968) and Glyn Daniel (1950, 1968) mention any women in Americanist archaeology. And as Cynthia Irwin-Williams (1990:6) noted, Gordon Willey and Jeremy Sabloff's exceptional and influential history of Americanist archaeology does not mention contributions from any women prior to World War I. Further, Alice Kehoe (1998) figured that for all periods, only 2 percent of all the citations in Willey and Sabloff relate to women. Moreover, one should note that this 2 percent figure depends on which edition of Willey and Sabloff one is utilizing—Reyman (1992:76) observed that virtually no women were mentioned in their first edition (1974), but by the second edition

(1980), in response to criticisms, Willey and Sabloff mentioned a very small number of women; no additions were made for the third and last edition (1993).

If 98 to 100 percent of the work in Americanist archaeology before 1940 was that of men, this emphasis would be reasonable, and Lurie's conviction that women in this field did not need to be "rescued" from obscurity would be apt. But I have found, for example, that in the graduate program at Harvard and Radcliffe by the mid-1920s and 1930s, between one-quarter and one-third of all archaeology graduate students were women (Browman and Williams 2013), and a similar distribution pattern seems to be reflected at the other five graduate institutions that offered the preponderance of doctoral degrees in anthropology. One-quarter to one-third: where did that estimate come from? it has not been previously reported. how apt is it? Not only does that particular estimate derive from my enumerations of graduate students in anthropology at Harvard and Radcliffe during this period, but it is also similarly reflected in research for this volume from my enumerations of women members of such influential groups as the Society for American Archaeology (SAA) from 1935 (its founding date) to 1941 as a proportion to all members. The results of these analyses plainly refute Lurie's statement above. Rather, as Mary Anne Levine (1994a:24) has observed, "One of the most difficult obstacles to the advancement of research into women in archeology is the persistent myth that women did not contribute to archeology until quite recently."

This pattern is not unique to archaeology or anthropology, but is rather broadly observed in the intellectual histories of Americanist sciences. Thus Margaret Rossiter (1982:xi), in her review of the contributions of U.S. women in science before 1940, reported that most women scientists "bordered, for a variety of reasons, on the invisible." And for the anthropological discipline, Nancy Parezo (1993:10) writes, "The history of anthropology has neglected women most often by pretending they have not been there." As Cattell (2006:18) notes for the genre of disciplinary history: "Often these works use descriptors such as erased,



hidden, invisible, marginal, muted, silenced, submerged, and subterranean because their common agenda is to bring the forgotten, the ignored, the excluded, and the silenced into the light.”

Nancy Parezo identified seven factors that she thought particularly contributed to this invisibility or marginality:

1. The structure of anthropological archaeology and the academy.
2. Education.
3. Ability to find employment.
4. Low status and nontenured positions.
5. Jobs in non-prestigious departments and institutions.
6. Teaching assignments.
7. Publishing, obtaining resources, and equal pay.

The following chapters focus on these issues and their changing relevance as the intellectual and social ambiance evolved.

Nineteenth-century researchers faced significant social barriers to securing recognition. For example, Matilda Coxe Stevenson was first told that unless her work was presented under her husband’s name, it would be ignored because she was a woman. Hence much of her work was published under her husband’s name, or at best, with her listed as the second author. Initially, she was given only an “honorary” position at the Bureau of Ethnology. But when her husband died, and the Bureau wanted to capitalize on her knowledge, they were finally forced to devise a means to employ her openly, and she became the first woman hired to conduct anthropology for the Bureau. One of the most egregious treatments of Stevenson’s contributions came to light when scholars tried to locate some of her earlier research. “Missing papers of Matilda Coxe Stevenson have been found in John P. Harrington’s papers in the National Anthropological Archives. He had removed her name from the copies, cut them up, and interspersed them with his own” (Parezo 1993:35n7).

My approach differs considerably from that of other recent volumes, however, such as Claassen (1994), Gacs et al. (1988), or White et al. (1999). The approach most frequently taken in the last two decades is that of a volume being published, such as the three just mentioned,

wherein the authors have, as the current cliché goes, cherry-picked the best-known earlier women archaeological scholars, or those women for whom they can readily find biographical data. For the most part, these volumes accordingly have focused on a dozen to a score of women. Hence there tends to be considerable overlap in coverage. These volumes also lack discussion of a significant number of women who contributed to the field.

In contrast, my approach in this volume is to be much more egalitarian and wide-ranging. The cohort that I identify involves over 200 named women in Americanist archaeology during this time frame (1865–1940). Moreover, I think it is ultimately essential to convey both the intellectual and social milieu for each researcher in order to adequately understand the context of the contributions. Disciplinary historiographers have gone far in developing general national intellectual frameworks in their publications. The social contexts have been largely overlooked. A significant part of this social context for women involved in Americanist archaeology is support and interaction with the broader community of other women in the same situation.

The traditional pattern in feministic archaeological historiography, unfortunately, is that where a woman who is perceived to have contributed more than her peers is identified and lionized while others who are a critical part of the same social and intellectual setting but who are less well publicized are systematically ignored. In a deliberate attempt to counteract that scholarly flaw or omission, I have made a painstaking attempt to identify the largest possible cohorts and communities of women involved in Americanist archaeology during each specified time period. As a result, the last two chapters in the present volume have evolved into what is often called a prosopographic approach, that is, a series of mini-biographical sketches of women for those decades, something I would rather have avoided, but it became necessary. I have been able to identify scores of women who were contributing significantly to Americanist archaeology. Unfortunately, many were unable to dedicate their entire careers to Americanist archaeology. However, if the women subsequently opted to leave, or were forced to leave,

archaeological research, coverage does not detail the specifics of their new career trajectory.

This entire volume is about gender. However, beyond a short discussion in my concluding remarks, I have not dealt explicitly with theoretical issues in gender studies, but rather have opted to provide documentation and evidence of the changes that women in Americanist archaeology faced during this period. The evidence provided here enriches the understanding of gender in science and in field situations, but avoids replicating what has been done well already, and instead focuses on the experiences of these women in Americanist archaeology.

### *1865 to 1900*

I now turn to some of the early commentaries, as well as historical analyses, of part for the three-quarters of a century covered in this book, emphasizing how they relate to opportunities and job placement for women. Because I have made the argument (Browman and Williams 2013) that there was a major pedagogic paradigm shift in Americanist archaeology around the beginning of the twentieth century, and that there was a second significant shift in academic training involving women students in the 1920s, I have broken this 75-year period into three broad temporal discussion units for this chapter: 1865–1900, 1900–1925, and 1925–1940.

Once again, perhaps unfairly, one can use Nancy Lurie’s commentaries as a ‘straw-woman’ foil. Lurie writes (1999/1966:7) about women of this period: “These women clearly accepted their role as women. Problems of ‘career vs. marriage’ and complaints about disadvantages and discrimination attendant upon being a woman were to come later.”

As a contrast to that view, one can also consider the statement about the difficulties for a woman doing scientific field research, made by Mary French-Sheldon (1894:737) at the World’s Congress of Representative Women, which was held as part of the 1893 Colombian Exposition in Chicago: “Woman needs must make great personal sacrifices, subdue her inherent sensitiveness, and meet the adverse criticism of not only

the opposite sex, but likewise the narrow-minded of her own sex. Too frequently she is denounced as unwomanly or fond of notoriety; her real motives are questioned.”

While Lurie may have captured the then “politically correct” perception of men and women in the late nineteenth century, arguing that women accepted the secondary political role they had been delegated, clearly women such as French-Sheldon did not agree. In addition, women notables including Susan Brownell Anthony, Elizabeth Blackwell, Elizabeth Cady Stanton, and Lucy Stone spoke against the cliché ideal of woman as a meek and subservient housewife. Or as Christine Ladd Franklin, later a member of Section H, Anthropology, of the American Association for the Advancement of Science (AAAS), wrote (1891:89): “Woman was looked upon as merely an adjunct to the real human being, man, and it was not considered desirable to give her any other education than what sufficed to make her a good housewife and an agreeable, but not too critical, companion for her husband.” Blackwell, Stone’s sister-in-law (and a relative of mine), reported that when she entered medical school as the first American woman in 1847 in Geneva, New York, the perception prevalent among many of the women in the community was “either that I was a bad woman, whose designs would gradually become evident, or that being insane, an outbreak of insanity would soon be apparent” (Blackwell 1895:70). Thus the view that all women of the late nineteenth century were complacent about the inherent biases does not reflect reality.

During the Civil War, women began working in jobs previously held by men. For example, U.S. Treasurer Francis Elias Spinner began hiring women as clerks. By 1875, when Spinner retired, women’s access to jobs as clerks in federal offices was securely established (Sizer 2004:12). Women also took over the jobs in general education teaching; by the end of the nineteenth century, women became the majority in the teaching profession jobs in primary and secondary education. By the 1870s, women were also employed in factories in greater numbers. This opening of opportunities for women is one characteristic, and possibly one cause, of the period known variously as the Progressive Era, Gilded Age, or American Renaissance (approximately 1870 to 1905). This was

a period of new zeal for reforming social, political, and economic structures, partly as a response to the rapid growth of industrialization that followed the end of the Civil War.

Only some of the women who contributed to this Progressive Movement did so as paid professionals. Most careers remained closed to women, except medicine, education, and social work. A significant portion of efforts expanding women's presence outside the home depended heavily on unpaid support from thousands of nonprofessional or avocational women working through social and philanthropic organizations (Curry 2004:24).

One of the outcomes of this period of particular relevance is that women were beginning to obtain access to more specialized advanced education in both public and private colleges by the late 1870s and 1880s. After retiring from her position as the second president of Wellesley College, Alice Elvira Freeman Palmer wrote an insightful review (1893:122) of the higher educational opportunities available for women. After having identified three types of educational programs for women extant in 1889, and reviewing the strengths and weaknesses of the first two—coeducational colleges and all female colleges—she observed that while “there are colleges for women which closely approximate in aim and method the colleges for men,” many of the women's colleges were somewhat stultifying, primarily training women for traditional roles. But she also saw intriguing potential for the third type, “the ‘annex,’ a recent and interesting experiment in the education of girls, whose future it is yet difficult to predict” (121).

The biases against women attending college were quite strong. In her 1891 book, *Woman's Work in America*, May Wright Sewall catalogued nine types of arguments raised against educating women, and particularly in coeducational institutions, by professors, college presidents, and the general public (Sewall 1891:78):

- a. Women are mentally inferior to men, and therefore their presence in a college will inevitably lower the standard of its scholarship.

- b. The physical constitution of women makes it impossible for them to endure the strain of severe mental effort. If admitted to college they will maintain their position and keep pace with men only at the sacrifice of their health.
- c. The presence of women in college will result in vitiating the manners, if not the morals, of both men and women; the men will become effeminate and weak, the women coarse and masculine.
- d. If women are admitted to college, their presence will arouse the emotional natures of the men, will distract the minds of the latter from college work, and will give opportunity for scandal.
- e. The intimacies of college life will result in premature marriages.
- f. Young men do not approve of the collegiate education of women; they dislike to enter into competition with women, and if the latter are admitted to our college it will result in the loss of male students, who will seek in colleges limited to their own sex, the social life which cannot be furnished by a co-educational institution.
- g. A collegiate education not only does not prepare a woman for the domestic relations and duties for which she is designed, but actually unfits her for them.
- h. Colleges were originally intended for men only, and the wills of their founders and benefactors will be violated by the admission of women.
- i. Whatever the real mental capacity or physical ability of women, so fixed is the world's conviction of their inferiority, that colleges admitting them will inevitably forfeit the world's confidence and respect.

The Annex variant that President Palmer of Wellesley College referred to in her comment above provided yet another strategy for education of women in the late nineteenth century. This Annex experiment evolved into current elite schools such as Radcliffe and Barnard. Because several of the women who entered into Americanist archaeology came from this educational variant, it is useful to look at it in greater detail.

Palmer utilized what was to become Radcliffe College as her example of an Annex. Beginning in 1874, at the request of the Boston Woman's Educational Association, Harvard College began offering examinations for young women (usually in their late teens), based on testing procedures started at Cambridge University. Upon passing the tests, the candidate received a certificate "with distinction" or "with the highest distinction" from Harvard (Woody 1929, 2:305). Initially, once the women passed these exams, Harvard provided nothing more. After the examination procedure had been in operation for four years, however, several of the college faculty, along with this group of concerned Boston women, suggested that basic courses already being offered at Harvard College for men should be repeated systematically for women.

Consequently, the School for the Collegiate Instruction of Women, more colloquially called the Harvard Annex, was formally opened in 1879, with a list of courses by 37 instructors, and with 27 students enrolled that first year. (One woman studied in Europe, and another withdrew because of the commuting distance from Cambridge, resulting in the final total of 25 students mentioned in the quote by Higginson below). In the second year, 42 women enrolled (Higginson 1880b:369; Woody 1929, 2:307). The then president of Columbia College in New York City, Frederick A. P. Barnard, observed in 1879, "To the success of the limited experiment of the Harvard Annex, so-called, may be traced the remarkable change in public opinion in regard to the university education of women" (quoted in Reed 1895:619). The Columbia Annex, which became Barnard, began a decade later, created in memory of Frederick Barnard, who had long advocated admitting women to Columbia.

A thumbnail description of the first year's operations and rationale of the Harvard Annex was given by Thomas Wentworth Higginson, a member of the Harvard Board of Overseers, in the *Woman's Journal* in 1879.

The variety of studies shows a wholesome range of tastes, and the "electives" sought belong rather to the more difficult than to the easier courses offered. Several of the students are teachers, in active

employment, who can take but a single study. One student is a graduate of Smith College, who comes to Harvard for history and political economy; while the student of “quaternions” is a Vassar graduate, and a pupil of Professor Maria Mitchell. Another of the students spent a year or two at Wellesley; and another was for a time the principal of the Oread Institute of Worcester. Eight of the 25 reside in Cambridge, the family of one having removed there for that purpose. Among these eight are two daughters of Professor Longfellow, one of ex-Professor Horsford, and one of Mr. Arthur Gilman, the secretary of the “Annex.” There are two students from New York, one from Connecticut, one from Vermont, and one from Missouri. The recitations take place in some cases in the houses of the professors, and in other cases at rooms hired for the purpose by the superintending committee.

To my mind, the great merit of the whole enterprise is not so much that it does something to remove the stigma of one-sidedness that has so long rested on Harvard, as that it is a weight thrown into the scale of thorough training, so far as it goes. In respect to the education of young women, I confess a strong sentiment against the whole race of “summer schools” and against all courses of winter lectures, however good, not followed by systematic examinations. All these may be pleasurable excitements or useful stimulants, but what is peculiarly needed is thorough training. To study somewhere and under some good teacher some one thing, to study it accurately and faithfully, and to be tested by some sort of examination afterwards—this seems to me to be the beginning of education. I confess that such exercises as those at Concord last summer, for instance, seemed to me to be likely only an injury to untrained and crude minds, however pleasant and profitable they might be to those already disciplined. American men and women are already too willing to believe that they can take in the most difficult study at a glance, or develop it out of their own internal consciousness, or by listening to “conversation”; what we need is to begin with mental discipline, always softened and enriched, of course, with intellectual enthusiasm. As a step in this direction, the new Harvard courses look very promising.



Higginson was one of a dozen speakers who provided five weeks of lectures to the public by subscription at the inaugural sessions in July and August 1879 at the short-lived (1879–87) “Concord Summer School of Philosophy.”

In 1889, Alice Palmer gave a more detailed description of how the Annex had developed in the intervening decade (it did not become “Radcliffe College” until 1893). Full-time tuition at the Annex was higher than at Harvard College—\$200—or \$50 more than the rate of \$150 then charged Harvard male students, making attendance at the Annex quite expensive (Reed 1895:614–15).

Only a few cases [of annexes] exist, and as the Harvard Annex is the most conspicuous, by reason of its dozen years of age and nearly 200 students, I shall describe it as the typical example. In the Harvard Annex groups of young women undertake courses of study in classes whose instruction is furnished entirely by members of the Harvard Faculty. No college officer is obliged to give this instruction, and the Annex staff of teachers is, therefore, liable to considerable variation from year to year. Though the usual four classes appear in its curriculum, the large majority of its students devote themselves to special subjects. A wealthy girl turns from fashionable society to pursue a single course in history or economics; a hard-worked teacher draws inspiration during a few afternoons each week from a famous Greek or Latin professor; a woman who has been long familiar with French literature explores with a learned specialist some single period in the history of the language. Because the opportunities for advanced and detached study are so tempting, many ladies living in the neighborhood of the Annex enter one or more of its courses. There are consequently among its students women much older than the average of those who attend the colleges.

The Business arrangements are taken charge of by a committee of ladies and gentlemen, who provide class-rooms, suggest boarding-places, secure the instructors, solicit the interest of the public—in

short, manage all the details of an independent institution; for the noteworthy feature of its relation to its powerful neighbor is this: that the two, while actively friendly, have no official or organic tie whatever. In the same city young men and young women of collegiate rank are studying the same subjects under the same instructors; but there are two colleges, not one. No detail in the management of Harvard College is changed by the presence in Cambridge of the Harvard Annex. If the corporation of Harvard should assume the financial responsibility, supervise the government, and give the girl-graduates degrees, making no other changes whatever, the Annex would then become a school of the university, about as distinct from Harvard College as the medical, law or divinity schools. The students of the medical school do not attend the same lectures or frequent the same buildings as the college undergraduates. The immediate governing boards of college and medical school are separate. But here comparison fails, for the students of the professional schools may elect courses in the college and make use of all its resources. This the young women cannot do. They have only the rights of all Cambridge ladies to attend the many public lectures and readings of the University.

The Harvard Annex is, then, to-day a woman's college, with no degrees, no dormitories, no women instructors, and with a staff of teachers made up from volunteers of another college. The Fay House, where offices, lecture and waiting rooms, library and laboratories are gathered, is in the heart of old Cambridge, but at a little distance from the college buildings. This is the centre of the social and literary life of the students. Here they gather their friends at afternoon teas; here the various clubs which have sprung up, as numbers have increased, hold their meetings and give their entertainments. The students lodge in all parts of Cambridge and the neighboring towns, and are directly responsible for their conduct only to themselves. The ladies of the management are lavishing in time and care to make the girls' lives happy and wholesome; the secretary is always at hand to give advice; but the personal life of the students is as separate and independent as in the typical co-educational college.

Manifestly, the opportunities for the very highest training are here superb, if they happen to exist at all.” (Palmer 1893:122–26)

While the emergence of the Annex (later referred to as a Coordinate) college system provided new educational possibilities for women, this development did not translate immediately into the entry of significant numbers of women into areas such as Americanist archaeology. Prior to the professionalization of the discipline, which began in the 1890s (Browman and Williams 2013), when there were as yet no degree granting institutions in anthropology or Americanist archaeology, there were a small number of both female and male avocational researchers who were making significant contributions to natural science disciplines. But when official university training began, because of the historical accident of the first success of anthropological degree programs at places like Harvard and Columbia—restricted male domains at the undergraduate level at the beginning of the twentieth century—professional anthropology changed to become the almost exclusive province of men for many years, with women only slowly reemerging as serious contributors.

My topical net at first identified more women involved in archaeological venues in this time frame, but as noted, I have opted to focus only upon Americanist archaeology. Male researchers in classical philology, which included classical archaeology, usually felt that the only relevant archaeological cultures worthwhile studying were those that led to European culture, that is, Greek, Roman, and Egyptian archaeology (Browman and Williams 2013). I found that it was not uncommon for women with classical archaeological interests at schools such as Bryn Mawr, Vassar, Wellesley, and the like, to obtain positions teaching classical studies on Greek and Roman topics and thus also utilize these positions to support their own archaeological researches. But this was not an option open to women interested in Americanist archaeology. Hence while Victorian age interest in Greco-Roman studies led to significant development of classics departments in the private women’s colleges, Americanist archaeology developed much later at those schools. Consequently there was a tendency prior to World War I for women to find

both training and opportunities more readily in classical archaeology. Thus the trajectories of both male as well as female archaeologists in classical archaeological studies were significantly different than those of Americanist archaeology and are not considered in this volume.

There were, to be sure, a few outstanding male supporters of women's entry into social science venues as well as those in classical studies. For example, Frederic Ward Putnam of the Peabody Museum at Harvard lectured to women in the Harvard Annex and was instrumental in recruiting of a number of them into Americanist archaeological activities through other mechanisms. Many of the women mentioned in chapter 1 interfaced with Putnam, who provided them moral and financial support, and encouraged them in several other ways. In his position as general secretary of the American Association for the Advancement of Science (AAAS) from 1873 to 1898, Putnam was active in recruiting women to membership in the AAAS, including at least two dozen directly or indirectly involved with Americanist archaeology. Additional details on Putnam's encouragement of women's entry into academia and research can be found in Browman 2002.

Even with such helpful encouragement and expanding opportunities, however, social and economic contexts resulted in far fewer women conducting archaeological field and lab research during the period between the Civil War and World War I, than women who aided archaeology instead by serving as financial patrons and philanthropists, acting as conservationists and preservationists, conducting support work as museum personnel, and working in related capacities.

### *1900 to 1925*

In general, for anthropological archaeology, as Lyn Schumaker (2007:278) observes, history in the twentieth century has been the story of women's entry into the discipline. In part this is because the beginning of the century was characterized by the evolution of more egalitarian educational systems in North America, which admitted more women and other members of previously excluded groups onto the lower rungs of

professional ladders, although in some cases, it took a protracted time for this to permeate all educational specialties.

For sociocultural anthropology, women fieldworkers benefited from the assumption that they could gain greater access to women's lives in other cultures in the research situation than could men. Thus men were urged to include their wives in fieldwork. The assumption of such differential access did not carry over to recruiting women for archaeology, however.

Paid professional service careers were progressively more culturally acceptable as an option for women by the first quarter of the twentieth century. The results of this shifting pattern were documented, for example, in the data collected by Mabel Newcomer and Ruth Hutchinson (1939) in an employment survey conducted among Vassar alumnae for the four decades from 1892 to 1931. In comparing graduates of the Vassar classes of 1892–1901 with the classes of 1917–26, Newcomer and Hutchinson found that at least occasional participation in paid occupations had increased from 52 to 67 percent for the married group and from 86 to 89 percent for the unmarried group. Further, comparing the classes of 1892–96 with 1922–26, they found that the occurrence of alumnae involved in social work and business had increased from 24 to 57 percent for married women and from 21 to 55 percent for unmarried women (1939:313, 316). Thus careers and paid employment outside the home were increasingly more socially and culturally acceptable alternatives.

Also contributing to the growing involvement was the overall expansion of public interest in the pre-Columbian heritage of the Americas. Hulda Hobbs (1946a:85) noted a significant increase in southwestern archaeology because of the decoration of “Indian dens” in New York, which had become “quite a fad in the eastern cities,” as detailed in an article in the local Santa Fe *New Mexican* in 1900. This is part of what Elizabeth Hutchinson (2009:3) defined as the “Indian craze,” a widespread passion for collecting Native American art, often in displays called “Indian corners,” which was promoted by illustrated magazines as well as department stores. Magazines such as the *Papoose*,” owned by the Hyde Exploring Expedition, featured the collections of Joseph

Keppler, one of Harriet Converse's group (see chapter 1) in their March 1903 issue. Just as antiquarianism and interest in collecting Greco-Roman and Egyptian artifacts in order to decorate the sitting rooms of British and French upper-class manors in the nineteenth century helped spur the growth of classical archaeology in Europe, so too did the increasing demand for artifacts and curios for Indian dens in the United States help provide jobs for collectors and archaeologists in the Southwest and elsewhere in the Americas.

The expansion of male researchers into regions such as the Southwest in the first quarter of the twentieth century is fairly well known and documented. The number of women involved in regional archaeology grew apace as well. This growth was also reflected in the college environment. In an unpublished news item that Frederic W. Putnam sent in 1906 to Frederick Hodge, who was then editor at the *American Anthropologist*, entitled "Anthropology at Harvard," Putnam reported the establishment and development of two undergraduate anthropology groups: (a) the Harvard Anthropological Society, founded in 1898, with 40 Harvard College male members in 1906, whose operation was described as follows: "During the college term, meetings are held for the presentation and discussion of papers and the review of the current anthropological literature. Two or more public lectures by eminent anthropologists are given each year in one of the Harvard lecture halls under the auspices of the society," and (b) the Radcliffe Anthropological Club, founded in 1903, with 25 Radcliffe College female members in 1906: "This club is conducted on the same general principles as the Harvard Society, except that no public meetings are held" (Putnam archival file, HUD 3146).

Among some male practitioners during this period, there seemed to be an implicit awareness of the awkward position of women in archaeology. For example, in 1918, Frederick H. Sterns, who had just finished his doctoral work at the Peabody Museum of American Archaeology and Ethnology at Harvard, penned a ten-page manuscript, "Women of Scientific Expeditions." In it he listed the women he knew who were involved in anthropological fieldwork. Because Sterns was trained and

employed at the Peabody Museum, it is not surprising that his study focuses mainly on Harvard-related cases of women conducting research, and that most of the women he was aware of doing fieldwork were in Americanist archaeology. His study identified 16 married women (for the most part wives of male archaeologists) and nine single women actively involved in anthropological fieldwork. Many of the married women, as discussed in chapter 2, actively participated in excavations and analyses. While they also helped their husbands with the final publications, they were less often credited as equal contributors. Of the single women that Sterns listed, four did fieldwork in classical archaeology, three were in Americanist archaeology, and two were mainly in sociocultural fields.

One of the putative indications of lessening discrimination against women in the discipline than earlier was the fact that the Anthropological Society of Washington (ASW), which had originally explicitly excluded women (see McGee 1889a, 1889b), finally asked the Women's Anthropological Society of America (WASA), also of Washington DC, to join them as members in 1899. However, this suggested merger was also at the time that the men's ASW publication, the *American Anthropologist* (old series), was in financial difficulty. To attempt to solve that problem, various ASW members joined with other regional anthropologists in exploring the possibility of establishing a larger entity, which would then have a sufficient membership to support a journal. This was the genesis of the American Anthropological Association (1902).

But what research reveals of the events in 1899, according to Smithsonian Institution anthropologist Clifford Evans, who gathered recollections of Washington old-timers in a 1958 memo, was that when the men's ASW invited the women's WASA to join them in 1899, it was almost solely because of the expected surge in financial support which would be derived from the newly transferred and incorporated WASA members. It was hoped that this expanded resource base would then allow the men's ASW to maintain their control of the *American Anthropologist*. "Thus it was the women's financial rather than intellectual resources that were valued" (Parezo 1993:56).

Despite the general increase in paid employment options for women,

securing appropriate employment was a continual problem for researchers. Ellen Fitzpatrick (1990:72–73) observed that turn-of-the-century University of Chicago social scientists were willing to train women PhDs, but not to hire them. In this regard, Chicago did not differ from most American universities. Fitzpatrick (1990:73) includes a revealing quote from Harry Pratt Judson, head of the Political Science Department at Chicago, in a letter sent to Madeleine Wallin, who gave up her teaching post at Smith College for marriage in 1896: “that no avocation for a good woman is higher than being a good wife for a good man—and it is my notion that only abnormal women think otherwise . . . higher education . . . is intended to make a woman more of a woman.”

Walter Thomas Woody’s 1929 *History of Women’s Education in the United States* includes a summary of the 1921 American Association of University Professors’ report on the status of women in colleges and universities, a document mentioned by several historical syntheses, but usually with little explicit detail. That report surveyed 176 American colleges. The 104 coeducational institutions had what seems to be a poor representation of women faculty, with 1,646 women out of 12,869 professors, or only 13 percent. On the other hand, the 29 single-sex male schools had only 2 women faculty out of nearly 2,000 professors, or a virtual absence of women. As might be expected, the ratio for women was more favorable at single-sex female schools. For the 14 major women’s colleges, the study reported 75 percent of the faculty members (748 out of 989) were women (Woody 1929, 2:329; Fitzpatrick 1990:73). But when it came to the number of female undergraduates enrolled in institutions of higher education, the percentage is significantly greater: 31 percent of the students for all types of colleges and universities were women, as contrasted to only 15 percent of the faculty.

The U.S. Bureau of Education statistics for 1916–1918 are very similar: in coeducational departments 79 percent of the instructors were men; in professional schools 98 percent of the teaching staff were men, and altogether 81 percent of all teachers in universities, colleges and professional schools were men (Fitzpatrick 1990:72). Some history of science researchers have chosen to emphasize the fact that a quarter century



earlier, nearly 100 percent of the instructors in colleges were male, so that women had gone from a negligible presence to 19 percent of the college faculty overall, a major increase. Other studies note, however, that the fact there were nearly twice as many female college students as there were female instructors is still a dismal statistic.

The ratio of female to male graduate students increased rapidly as women students were allowed into higher education. In 1892–93 there were 3,081 men and 484 women graduate students. In 1900–1901 there were 4,883 men and 1,982 women graduate students, of whom 1,106 men and 295 women received master's degrees, and 312 men and 31 women obtained doctoral degrees. By 1909–10 there were 7,508 men and 2,991 women graduate students, and by 1919–20, there were 9,837 men and 5,775 women graduate students (Woody 1929, 2:337–38). Thus in the quarter century from 1892 to 1918, the number of female graduate students increased from 13.5 to 37 percent, but as noted, this increase was not reflected by a comparable increase in the percentage of women in faculty positions.

Even as the number of job openings increased, equality did not necessarily follow. Positions available for women faculty members received a big boost during the First World War. Women had taken jobs during war emergencies, obtained jobs because the scarcity of men faculty who had left for better paying jobs, and been hired because of a new but belated realization of the capabilities of women (Woody 1929, 2:329). However, simply counting the number of women gives an incomplete or skewed picture of progress.

The hiring and promotion of women was much slower than that of men. For example, at the University of Chicago, the time from hiring to achieving full professor rank was usually nine years longer for women than men. And at 47 percent of schools in Woody's survey, no woman held a full professorship (Woody 1929, 2:330). Of the nine women who earned doctorates in social sciences during the first 15 years of graduate studies at the University of Chicago, not a single one secured a regular faculty appointment upon her graduation, while two-thirds of male classmates were so employed (Fitzpatrick 1990:72).

Financial compensation also reflected discriminatory biases. Women received pay equal to their male counterparts at 73 percent of the women's colleges and at 53 percent of the co-ed institutions, but other institutions "frankly admitted women are given a less salary and lower rank than men for the same work" (Woody 1929, 2:331). The reasons given to justify this bias were (a) the law of supply and demand, (b) the fact that traditionally families relied on men for support, and hence a man needed a job more than a woman did, and (c) the need to retain men in college teaching required men to be given salary sufficient for a living wage. Married women not only received lower pay but also were discriminated against in hiring situations, the argument given that they did not need a job because they had working husbands who could support them.

Nor were women treated as equals in other academic venues. Dr. Margaret Murray, who briefly was involved in fieldwork in the U.S. Southwest in the teens before devoting her career to Old World archaeology, attended her first meeting of the Anthropological Section of the British Association for the Advancement of Science in 1913 and found only one other woman in attendance. One of the men there, a colleague whom Murray knew well, indicated that he did not approve of her attending, saying, "There are many things in this world that a woman shouldn't know. I certainly would not permit you to attend one of my lectures" (Murray 1963:92).

### *1925 to 1940*

The position of women in archaeological endeavors improved in the period between the First and Second World Wars, although this did not immediately translate into more jobs. Among the changes were greatly expanded graduate training opportunities for women, as well as improved field funding. For example, Margaret Rossiter (1982:270) found that between 1920 and 1938, 59 out of 197 (30 percent) of the PhDs in anthropology were earned by women, and 10 out of 26 (38.5 percent) of the National Research Council Fellowships in anthropology were awarded to women.

Harvard University had the first successful undergraduate major and first functioning graduate program in anthropology in the country. There, from the beginning of the official anthropology program in 1890, Frederic W. Putnam and his staff had also offered a limited range of undergraduate courses to women, first through the Annex and then through dual class offerings taught separately at Radcliffe and Harvard. But initially there was what amounted to a “glass ceiling”: limited access for women to training as undergraduates in the discipline, as well as severely limited access to graduate work and professional positions.

In the beginning, Harvard College did not permit (female) Radcliffe students to attend the same classes as (male) Harvard College students, but rather the anthropology staff was required to teach separate sections for the Radcliffe women. In the mid-1920s, there was a policy shift at Harvard. Now women undergraduate and graduate students were allowed to attend the same classes as male students, although at first in a rather tentative manner. For example, Doris Stone (1980:20) recollected: “In the 1920s, I was required to obtain permission from the president of Harvard to attend classes at the Peabody Museum, and with that permission came the warning that if my deportment was not entirely proper, my association with that austere building would be ended.”

During this decade, the numbers of both male and female graduate students in the discipline increased significantly at Harvard. By the end of the 1920s, 20–25 percent of the graduate class was female, and the combined enrollment of anthropology graduate students from Harvard and Radcliffe had increased markedly, averaging 25 to 30 students each year (Browman and Williams 2013).

This growth in opportunities for female students was observed at other institutions as well. For Columbia University, all but two of Franz Boas’s doctoral students before 1920 were men, but from 1920 to 1940, the pattern changed, with 20 men and 20 women obtaining their doctorates (Parezo 1993:35). At the American School for Prehistoric Research, 29 of 79 students (37 percent) were women between 1921 and 1938, and for the 1931 season the field school was directed by Charlotte D. Gower (Bricker 2002:281–83). Carl E. Guthe of the University of Michigan, in

his 1952 review of Eastern U.S. archaeology, noted a dearth of female archaeologists before 1925, but a significant increase during the 1930s.

Surprisingly, Edgar Lee Hewett's lists for School for American Research (SAR) field schools in Chaco Canyon, New Mexico, for the 1929–34 period, identified 60 percent of the students, 35 out of 58, as women (Mathien 1992:112–13). This latter figure must be viewed with some care, as one of the women involved, Winifred Stamm Reiter, reported that Hewett was under pressure to demonstrate the success of the program, and thus he padded the class list. Reiter tells us that Hewett listed as students at the field school individuals who were members of the SAR Managing Board; teachers from public schools who needed to update their work to maintain their certificates; family friends; associates on vacation; donors to the SAR, etc. (Mathien 1992:108–9).

Nevertheless, as discussed in chapter 3, a significant number of women who continued in archaeology obtained their first field training from Hewett's summer excavation projects, and this policy continued after his retirement. When Donald Brand replaced Hewett as department chairman and field school director, the 1936 University of New Mexico field schools maintained the tradition of substantial numbers of women: 75 percent or 35 of the 47 students listed were female (Joiner 1992:58).

However, even though there were sizeable numbers of women on several of the Chaco Canyon field school projects in the early 1930s, men continued to dominate the supervisory positions, and the men were also more successful in securing graduate degrees. Jonathan Reyman reports (1994:86) that 57 percent of the men at the University of New Mexico anthropological graduate program then obtained their MA degrees, and 30 percent went on to obtain PhDs, while only 26 percent of the women finished their MA degrees and only 11 percent went on to the PhD.

The new availability of archaeology as a career for women is nicely captured in the two short vignettes by Frances Emma Watkins, *My Experiences as a Field Archeologist* (1930) and *Archeology as a Profession for Women* (1931). Watkins had gone to a field school at Pecos directed by Alfred V. Kidder in the Southwest in 1929. These two commentaries were based primarily on her fieldwork later that season with two other female

students on an all-female-directed excavation project at Tecolote, New Mexico, for Kidder. Watkins wrote that “archeology as a profession for women is new, at least in America” (1930:13), and explained the lure of the profession by asserting that “women are, I think, peculiarly adapted to these new methods of excavation and research” (1931:175).

There is great opportunity for specialization in archeology, for there are many branches and subdivisions. . . . The best preparation is to take as much general work as possible, and then with such a background choose your specialty. One thing is pretty certain—a college education is practically required, with a basis of scientific courses: anthropology, of course, and the scientific languages (French and German), Spanish if you intend to work in America; then geology, geography, and zoology, and history.

Field training, formerly considered unnecessary and not as yet a requirement, is becoming more and more important.” (Watkins 1931:177)

Dorothy Thomas wrote about the new job opportunities also available to women in the museum field, but with a slightly different spin. In her article in the *Independent Woman*, the journal of the National Federation of Business and Professional Women’s Clubs, Thomas noted that the director of the Brooklyn Museum believed “that women are especially suited to museum work by their love of the beautiful, their adaptability and their patience in detail work” (Thomas 1933:238). However, Thomas also reported that the museum personnel whom she interviewed indicated that to secure jobs women should not “hesitate to take voluntary positions first and prepare to spend years breaking into any responsible museum position,” and that even well-trained women should expect different levels of responsibility than men, because “men come as scientific experts and therefore demand more. They will not do routine work as women scientists have to” (239).

Thus in spite of the fact that more positions were potentially open to them, it was still difficult for women. Specialization was one option. Florence Hawley Senter Ellis told her student Florence Cline Lister

that the “only way to break into the discipline was to find some aspect of the fieldwork that men did not enjoy doing and then to become a specialist in it” (Lister 1997a:5). Ellis herself had developed a special skill, dendrochronology, as a strategy to ensure a place in archaeological research. Mary Ann Levine (1994b:27) noted that geographic specialization was also important. She saw Frederica de Laguna’s focus on the Arctic prehistory and Dorothy Cross Jensen’s fieldwork in New Jersey, for example, as being in “virtually uncontested geographic areas, as creative responses to marginalization.”

Another option that women perceived as an entry into archaeology was marriage. Lister writes that Ellis also told her: “Your best bet is to marry an archaeologist.” Lister reports that she, as well as her archaeological classmates, such as Gretchen Chapin (who married Alden Hayes) and Carolyn Miles (who married Douglas Osborne) did just that (Lister 1997a:7). Richard Woodbury (1993:19) had a similar take on the situation, reporting that in 1927 there were five husband-wife teams working in the Southwest, “virtually the only way a woman could engage in field archaeology in the 1920s.”

Eleanor Bachman Lothrop (1948:227–28) and Florence Cline Lister (1997a:19) categorized their fellow women graduate students who married archaeologists in three typological clusters. They identified one group of wives who tried to “out-macho” the males in behavior and mien; a second group who might be viewed as dilettantes, characterized as being shallow with little or no continuing interest in archaeology; and a third group, to which they both assigned themselves, consisting of women who accepted various male biases, but still tried to be active and congenial collaborators. My female colleagues tell me that little has changed with respect to these three clusters up to the present day.

Certainly, being the wife on a husband-wife team had its costs. Emma Reh Stevenson, one of the founders of the SAA, noted in a letter to a colleague: “It’s awful to be a woman and married because you really cannot have any plans of your own” (Stevenson to Davis, Record Unit 7091, box 108, folder 1, 1926). Cynthia Irwin-Williams (1990:13), writing about the team of Winifred and Harold Gladwin, noted that while

their definitive ceramic analyses were jointly authored, most individuals (even Willey and Sabloff in their seminal history of the discipline) refer to the Gladwins' joint significant methodological contributions as just "his." Lyn Schumaker (2007:282) suggested that a woman's status as a wife also adversely affected her evaluation both by her male and female colleagues. Wives were often expected to do the tedious jobs. Barbara Kidder Aldana noted that her mother, Madeleine Kidder (Alfred V. Kidder's wife) "worked tirelessly cleaning, mending and cataloguing the specimens," as well as doing ceramic analysis while being essentially unrecognized (Aldana 1983:248). Donald Lehmer (1948:100), in his report on the 1940 Works Progress Administration (WPA) project in the Forestdale Valley, east-central Arizona, thanked his first wife, archaeological graduate student and collaborator Mary Scanlan Lehmer, for her help in hauling water, doing the marketing and cooking, and running bilingual poker games in the evening, but was mute in terms of acknowledging her significant assistance in excavation and write-up.

The cultural context of the United States prior to World War II, wherein men were still assumed to be the breadwinners, had an adverse impact on the ability of women archaeologists to advance as well as to find jobs. During this epoch, one of the standard practices for several prestigious universities (such as Chicago or Columbia) was the requirement that the doctoral dissertation must be published in order for the degree to be awarded. The assumption, deriving from the northern European heritage of higher education, was that the dissertation effectively was to be one's scholarly *Magnum Opus*.

Anthropology departments were perennially short of funds, and often decided that they could not afford to publish the dissertations of all of their candidates. Thus one of the founding "mothers" of the SAA, Regina "Gene" Weltfish finished her degree research and successfully defended her dissertation in 1929, but was not granted her degree until 1950, when Columbia University eliminated its publication requirement. Weltfish estimated that it would have cost her \$4,000 to publish the dissertation herself, and she could not afford to do so. (For the basis of this estimate, see chapter 3.) Her department's position was the standard

“men have greater need” in terms of supporting families, and therefore the department devoted its limited publishing funds to support publication for their male candidates. Thus, without publication, the PhD could not be awarded; without a PhD in hand, Weltfish could not secure a tenure track job; and hence she served as a nontenure track lecturer at Columbia from 1935 to 1953 (Pathe 1988:373; Price 2004:110).

Florence Hawley Ellis was one of the few fortunate women in this respect. Because Edgar Hewett wanted her to conduct dendrochronological studies at the University of New Mexico, he found the money to publish her dissertation so that she could receive her doctorate from Chicago before she began to teach (Parezo 1993:347, 366n9). But even publication was not always straightforward. For example, Charlotte Gower received her University of Chicago PhD in 1928, but her book manuscript based in her dissertation research, which was submitted to University of Chicago Press in 1935, was misplaced at the press and not found until the late 1960s, so her 1935 work was not published until 1971, long after her retirement (Lepowsky 2002:126).

In the 1920s and 1930s, a number of women waited years to officially receive their doctorates. Official dissertation publication was often the culprit. Many women simply gave up, when faced with this impediment, and never secured the degree. Obviously, because many U.S. universities were trying to upgrade their faculties by requiring doctoral degrees of their professorial staff, this lack of degree prevented a significant number of women from securing tenured positions and advancing to upper-level academic ranks.

Many women involved in postgraduate archaeology were carried on the books as research associates. These were in primarily courtesy appointments that offered institutional affiliation to otherwise unemployed or underemployed researchers, categories that defined a significant number of women archaeologists (Levine 1994b:28). This situation was no doubt exacerbated by the fact that, as Rossiter (1982:272) argues:

Anthropology presents an extreme case of this dependency on foundations and fellowships during the 1920s and 1930s. Since it was



a small field and had few teaching positions available, most of its younger women did important work and built whole careers on little more than a series of temporary fellowships from the NRC and SSRC. In fact, there seems to have been a tendency, in this field at least, to give the fellowships to the women to “tide them over” while the few jobs available went to the men.

She notes (1982:139) that particularly Franz Boas was successful in being able to transform the field’s lack of jobs into a noble, self-sacrificing cause of great appeal to women graduate students at Barnard/Columbia.

While women were being placed in the lectureship and research associate positions, revisions were being made in the tenure system which negatively affected their job security. In an attempt to standardize the treatment of junior faculty positions, administrators at Harvard devised a new tenure system in the mid-1930s, with a seven-year review period limit before granting tenure. Thanks to the actions of the American Association of University Professors (AAUP), this tenure procedure became a standard system nationwide by 1941 and is still mainly the norm today. However, as it was being put in place in the late 1930s and 1940s, an extremely high proportion of its victims were the long-serving women who had been relegated to nontenure track or lower academic levels, and thus had long passed the seven-year limit without receiving permanent appointments (Lepowsky 2002:155, Rossiter 1982:194). The AAUP regulations argued that this was exploitative; that the institutions either had to grant the women tenure or let them go. Not surprisingly, in many cases the women were simply terminated.

In spite of the upsurge in the number of women in Americanist archaeology in the 1930s, this shift was very slow to be appreciated by the men. There were significant numbers of women doing archaeology during this period who became members of the Society for American Archaeology. But the men in the field seemed rather unaware of, or perhaps unappreciative of, this change, if nominations to fellow status at the SAA and acceptance of papers for the annual meetings are appropriate statistics.

To be named a fellow of the SAA, an individual had to have engaged in scientific research in American archaeology and to have published upon this research in recognized scientific media. At the first meeting of the SAA held in December 1935, there were only four nominations of women out of 42 individuals put forward for fellows (Guthe 1936a:310; Strong 1936:315). At the second annual meeting in 1936, six fellows were nominated, including one woman; at the third annual meeting in 1938, only one woman out of 17 nominees; and at the fourth annual meeting in 1939, and fifth annual meetings in 1940, 22 fellows were nominated, but no women were listed among the group for these two years (Guthe 1936c:290, 1938:42, 1939:39, 1940:98). The papers presented in these early years were only just slightly more representative of the actual female membership: at the first annual meeting in 1935, 1 paper out of 8 was presented by a woman; at the second, 5 out of 17; at the third, 3 out of 23; at the fourth, 2 out of 20; and at the fifth, 3 out of 21 papers, or a total of 16 percent of the papers presented during the first five years were by women (Guthe 1936b:316, 1936c:291, 1938:43, 1939:38).

There was considerable resistance among some of the male archaeologists to accept women students or to accept them as equals. Arguments ran the gamut, but among those most frequently voiced was the old saw that the degree that women students were seeking was the “Mrs.” and that their physical abilities were suited only for lab work, not fieldwork.

In fieldwork, for example, Alfred V. Kidder thought women were “unreliable” members of crews because they got married, but he was not worried about men marrying because then their wives could serve as unpaid field assistants (Babcock and Parezo 1988:v; Reyman 1994:89n4). Alfred Kroeber was reluctant to admit women to Berkeley’s PHD program, because he “felt they were not interested in archaeology, but only in male archaeologists” (Lepowsky 2002:131n1). As an aside, it is interesting to note the tendency of senior male archaeologists (such as Alfred Kroeber and John Harrington reported in later chapters) to conduct affairs with younger female students while at the same time suggesting that women were only looking for husbands. There is a certain predatory duplicity to this theme that would be intriguing to investigate; it is

a logical counterpoint to the “stress in marriage theme” explored later in my concluding remarks.

At Harvard, Ruth Otis Sawtell Wallis reported that Alfred Tozzer and Earnest Hooton “asserted that most young women in graduate work abandoned it if they married. They added, however, if she had serious intentions to study, they would help her in every way” (Collins 1979:85). And Doris Zemurray Stone wrote about the same issue concerning women working for advanced degrees in anthropology at Harvard, recollecting that “I was advised against studying for a PhD. Women simply weren’t encouraged to go that far, particularly in anthropology. . . . And eventually, when I returned to the Peabody Museum for a visit in the 1940s, one of my former professors, who also thought little good came from educating females because they soon grew up and were married, invited me to speak at a weekly tea gathering at his home” (Stone 1980:20–21). Notably, both Stone and Wallis made it clear that once they decided to proceed and demonstrated their seriousness, the male faculty at the Peabody Museum were extremely supportive of their work.

For the most part, men were oblivious to the fact that they were discriminating against women. A typical case might be James B. Griffin, widely recognized as the unofficial “dean” of Eastern American archaeology for many years. For the fiftieth anniversary of the SAA, he wrote a retrospective about the founding of the society, identifying one of his fellow cofounders, Ellen S. Spinden, merely as the wife of Herbert J. Spinden (Griffin 1985:266). Ellen had her MA from Harvard as well as advanced training in archaeology at the Peabody Museum, and she was actively working in the field and publishing in Mesoamerican archaeology at that time, but all that registered with Griffin apparently was her marital status. Later, when Griffin was asked about the difficulties of being a woman archaeologist in the 1930s, he replied: “There is no question that it was difficult for a woman to become an archaeologist. It was also difficult for a man. That was true in the 1930s and has been true up to the present time.” He added, “The statement that anthropology was not ‘a proper occupation for a young lady’ in the 1930s is almost certainly true for the vast majority of American Christians, it wasn’t for

young men either, ‘proper’ or not” (Griffin 1992:7). I knew Griffin for many years as a wonderful, caring man, but when it came to the issue of historical discrimination against women archaeologists, he had his blinders on, it seems.

Women archaeology students were often required to endure demeaning situations to secure graduate training. As an undergraduate at Columbia University, Dorothy Louise Strouse Keur was delegated to fetch a bowl of soup every noon from the lunchroom and bring it to her professor’s (Edward Sanford Burgess) office. After graduation, Burgess offered her a job as lab assistant, but only if she agreed to return half of her salary each month so he could purchase prehistoric artifacts for the departmental museum (James 1988:181–82). Graduate student Clara Lee Fraps Tanner reported being used as a babysitter in the 1930 field season by Dean Byron Cummings instead of being allowed to excavate, the kind of treatment that ultimately resulted in her leaving archaeology (Hays-Gilpin 2000:92).

Nancy White has written, “It could be argued that the way women are traditionally socialized in our society makes them well suited for field and lab management,” and “Other studies have noted the female tendency to prefer/excel in perceived tedious tasks such as sorting lithic debitage” (1999a:14). This perception has been shared essentially for the duration of development of Americanist archaeology. For example, it was nearly a century ago that Clark Wissler argued that women were particularly well suited to lab work because it resembled housework (Babcock and Parezo 1988:4). This sentiment has been expressed in similar ways throughout a century and a half of Americanist archaeology.

Edgar Hewett might seem laudable because he enrolled so many women on his field crews in the Southwest, particularly in the 1930s. However, he did not encourage them to complete advanced degrees, and he had double standards about pay and duties. Not only did fewer of the women students there receive advanced degrees, but he also actively dissuaded them from pursuing doctorate work. Hewett had a limited budget, and he deliberately hired women because he could pay them less (Cordell 1993:220). He also gave them far less authority (men

were usually the field supervisors), and often assigned the women to lab work rather than letting them excavate. When University of New Mexico graduate student and museum employee Marjorie Lambert wanted to go to an eastern university to pursue a doctoral degree, Hewett would not let her. So Lambert decided that for museum work, a PhD was not important, and she quit her graduate studies at that point (Parezo and Hardin 1993:283).

The 1929 Tecolote, New Mexico, field project was one of the first where women held positions of authority. In 1929 Fay-Cooper Cole, Roland B. Dixon, and Alfred V. Kidder reported the distribution of the first national scholarship competition to be funded primarily through the aegis of the Laboratory of Anthropology in Santa Fe for fieldwork in archaeology, ethnology, and linguistics. There were 38 applications for 14 scholarships—15 in archaeology, 18 in ethnology, and five in linguistics, with 11 from women and 27 from men. Criterion #3 for awarding the money, however, was as follows: “That as there are at present open to women relatively few professional positions in anthropology, the number of scholarships granted to women should be limited. Furthermore, the conditions under which the investigations are being carried on during the summer of 1929 preclude the assignment of women to the ethnological and linguistic parties” (Cole et al. 1929:572).

Six awards were made in archaeology: three to women and three to men. Cole was reluctant to accept women students, as he noted in a letter written to Edward Sapir, chairman of the scholarship committee for 1930: “I should prefer not to have girls in the party, but if your Committee finds it necessary I will take two” (Sampson 1992:9n1). Thus Kidder ended up with the six archaeology fellowship students, and after training the group for a month at Pecos, he sent the three women to run all the work at the site of Tecolote, making it one of the first Americanist excavations where women were in charge of all operations.

The Depression, of course, had a significant impact on availability of jobs in the 1930s. But even when the Works Progress Administration (WPA) and other government agencies were desperate for trained archaeologists to supervise the projects, qualified women were generally

neither considered nor chosen. Cheryl Claassen (1999:92) identifies Dorothy Cross as perhaps the only woman to have a supervisory position. I will add the names of women such as Catherine McCann and Harriet M. Smith in my discussions here in later chapters, but it was indeed the case that WPA and other Depression era programs provided funding and fieldwork opportunities for a large number of male archaeologists, whereas women found extremely limited access to this employment sector.

Emma Reh Stevenson, one of the founders of the SAA, observed that being a woman at times had both aided and hindered her. In a November 7, 1927, letter to Frank Thone, a colleague at *Science Service*, she attributed some successes to the chivalry of men she had encountered, who went out of their way to help her, but also contrasted this to other situations where she had to convince people that as a woman, she was able to handle science (Record Unit 7091, box 108, folder 1). On the other side of the coin, however, she noted in a January 1929 letter to Watson Davis, another colleague at the *Science Service*, that she could use science as a mechanism to travel and do all sorts of unwomanly things while retaining the benefits of being a woman (Record Unit 7091, box 120, folder 3).

We should note, however, that it was not only male archaeologists who discriminated against female students. Theresa B. Goell, who excavated in Turkey in the 1930s and 1940s, wrote to her former professor, William Albright, May 21, 1953:

When I was younger I used to think that you imposed difficult conditions on women in archaeology, but I have become perhaps even stricter than you—because I can allow myself the luxury of discriminating against my sisters. I have made a rule that no wives can come along and make a nuisance of themselves and no young girls well-recommended who have not yet proven that they will remain in archaeology. One wastes too much energy with the disrupting idle wives and the dilettante young ladies. (quoted in Sanders and Gill 2004:496)

Not surprisingly the basic patterns identified continued for much of the rest of the twentieth century. Thus Melinda Zeder, in her review of American archaeologists at the end of the twentieth century, noted, “For though a higher proportion of younger women than men are going into academia overall, there is a persistent, if not increasing, tendency for women in academia to occupy nontenure track positions with more limited potential for advancement and job security. . . . Moreover, women continue to make greater personal sacrifices in their family lives to pursue careers in archaeology. They both marry less frequently and are more likely to delay, or forego, having children than are men” (Zeder 1997:2).

Nancy White observed, in writing about women in Southeast archaeology during this period (and later), “Most of the women in this book, for example, noted many other women in their field schools, often a predominance of women, not only as fellow students but also as help-mate wives, cooks, and lab assistants. Because of the undervaluing of their contributions, or perhaps shorter duration in the field because they then took up more traditional women’s roles, there surely are far more such ‘invisible women’ than we think” (White 1999a:21).

These assessments by Zeder and White provide one of the rationales and *raison d’êtres* of this volume, that is, to ferret out and bring the contributions of these invisible but meritorious women to light.